The "Flight" of Flying-fish.

I HAVE on frequent occasions (in the Mediterranean, the Red Sea, and the Indian Ocean) carefully observed with a field-glass (×8) the supposed "flight" of flying-fish, and have always concluded that the "leap and glide" theory is the correct one, with one or two modifications. Dr. J. McNamara, in NATURE for June 3, p. 421, cites five facts in support of the theory of true flight, but I may point out that all these five facts can be otherwise interpreted. Flying-fish undoubtedly leap out of the water and gain their initial impetus by tail action, and when out of the water the pectoral fins serve as planes. While gliding the fish can not only renew its impetus to a limited extent by an occasional flick of its tail against the crest of a wave, but, as your correspondent says, can also change the direction of its glide. I have, however, never observed a fish "come back in a direction opposite to the direction in which it set out," and I am tolerably certain that it could not do this without re-immersion in the water, unless perhaps a strong wind were blowing in this opposite direction. Flying-fish can certainly rise and fall during the glide, but this, as well as change of direction, can be easily explained by assuming inclinations of the planes of the fins—a very different process from actual "wing"-flapping sufficient to cause flight. The fins can, like those of most fishes, move on their bases, but I fail to understand how, in the absence of the required musculature, it can possibly be supposed that the fins show "rapid movement, as in the case of hovering flies and humming-birds." If seagulls can glide for hundreds of yards, rise and fall, and change direction without wing-flapping, why not flying-fish? In gliding the outlines of the pectoral fins naturally appear to be indistinct, because, compared with the rest of the body, the fins are thin and irregular in outline on their posterior edge.

Granting that the body can gain fresh impetus by an occasional flick of the tail against a wave-crest (and this can be easily seen to occur, and is certainly less difficult to understand than the initial tail action which enables the fish not only to emerge from the water, but also to acquire an impetus which carries it the greater part of its glide), and that the planes of the wings can be inclined, all the movements of flying-fish which I have observed are fully intelligible.

W. N. F. WOODLAND.

"Kismet," Lock Mead, Maidenhead, June 4.

As another observer of Nature at sea I must beg to differ entirely from Dr. McNamara's conclusions on the "flight" of the flying-fish.

(1) Turning at an acute angle can be brought about by an extra puff of wind, and indicates no power on the part of the fish.

(2) It is impossible for a flying-fish to flap its pectoral fins as a bird does its wings.

(3) The rise and fall over waves are due to the forcing up or lowering of the air immediately over the surface of the water.

(4) The impetus is quite sufficient to send flying-fish up to a height of 50 ft. or even more, and to extend the soar to 300 yards. They naturally flop about on deck until dead.

(5) It is quite possible (though I have never seen it) for the tips of the fins to be vibrated by the wind during flight.

The matter has been dealt with more fully in "Nature Notes for Ocean Voyagers," by Capt. Alfred Carpenter and myself, and also in the *Nautical Magazine* for May, 1894, and in the *Shipping World* for April, 1901. The late Capt. Cromie, at my request, made a series of very careful observations from torpedo-boat destroyers and submarines, and was most emphatic that they did not "*fly*."

As in many other interesting problems, the help of a super-kinema camera fitted with a telephoto lens would be of great service.

DAVID WILSON-BARKER.

Fellow-Workers.

IN NATURE for June 3, p. 416, Prof. D'Arcy Thompson refers to me and to my "fellow-workers" who helped me to bring our "hopes to fruition" in connection with the old malaria-mosquito business. My own memories remind me of seven years' almost continuous solitary labour, during which time my numerous "fellow-workers" had many opportunities, as good as mine or better, for doing the same work, but, oddly enough, did not use them; and it was not until I had solved the problem that they arrived on the scene in a body, fully armed with paper, pens, and cameras, and resolved "to join the victory group" at any cost. Prof. Thompson puts one of these gentlemen in the place of honour next to Pasteur—who, by the way, had little to do with the development of animal parasitology. The true history of the subject is given in my "Prevention of Malaria" (Murray), and still more trenchantly in Robert Koch's letter to me, dated February 10, 1901, and published in *Science Progress* for April, 1917.

But this is a detail; and I should like to thank Prof. Thompson for his kindly references to my medical verses, and for his interesting conspectus of the medical poets. Oddly enough, the day after it appeared in NATURE I lectured at the Royal Institution on "Science and Poetry," and upheld the thesis that a higher view of both will show how frequently and how closely they are connected. But honesty compels me to add that my own interest in medical matters is quite secondary, and a matter of duty rather than of predilection.

RONALD ROSS.

36 Harley House, London, N.W.1, June 4.

The Approximate Evaluation of Definite Integrals between Finite Limits.

(1) THE four-ordinate rule given in my letter published in NATURE of May 20, p. 354, viz.

$$\int_0^1 \mathrm{F}(x)dx = \tfrac{1}{4}\{\mathrm{F}(\tfrac{1}{10}) + \mathrm{F}(\tfrac{4}{10}) + \mathrm{F}(\tfrac{6}{10}) + \mathrm{F}(\tfrac{9}{10})\},$$

is obtained by dividing the range into two and to each half applying the simple two-ordinate rule,

$$\int_0^1 \mathrm{F}(x)dx = \tfrac{1}{2}\{\mathrm{F}(\tfrac{1}{5}) + \mathrm{F}(\tfrac{4}{5})\},$$

the parabolic or cubic approximation for two ordinates being

$$\int_0^1 \mathrm{F}(x)dx = \tfrac{1}{2}\left[\mathrm{F}\left(\frac{3-\sqrt{3}}{6}\right) + \mathrm{F}\left(\frac{3+\sqrt{3}}{6}\right)\right]$$

$$= \tfrac{1}{2}[\mathrm{F}(0{\cdot}2113) + \mathrm{F}(0{\cdot}7887)]. \quad . \quad . \quad (a)$$

(2) Closer approximations may be obtained by dividing the range into a greater number of parts and applying this rule to each, thus:

$$\int_0^1 \mathrm{F}(x)dx = \int_0^{\frac{1}{3}} \mathrm{F}(x)dx + \int_{\frac{1}{3}}^{\frac{2}{3}} \mathrm{F}(x)dx + \int_{\frac{2}{3}}^1 \mathrm{F}(x)dx$$

$$= \tfrac{1}{3}\left\{\int_0^1 \mathrm{F}\left(\frac{x}{3}\right)dx + \int_0^1 \mathrm{F}\left(\frac{1+x}{3}\right)dx + \int_0^1 \mathrm{F}\left(\frac{2+x}{3}\right)dx\right\}$$

$$= \tfrac{1}{6}\{\mathrm{F}(\tfrac{1}{15}) + \mathrm{F}(\tfrac{4}{15}) + \mathrm{F}(\tfrac{6}{15}) + \mathrm{F}(\tfrac{9}{15}) + \mathrm{F}(\tfrac{11}{15}) + \mathrm{F}(\tfrac{14}{15})\}$$

The following table shows for several functions the value of the integral and the approximate evaluations from two, four, six, and eight ordinates:

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$F(x)$	$\int_0^1 F(x)dx$	Number of ordinates used. 2	4	6	8
Semicircle $(x-x^2)^{\frac{1}{2}}$	0·3927	0·4000	0·3949	0·3939	0·3934
Quadrant $(1-x^2)^{\frac{1}{2}}$	0.7854	0·7898	0·7868	0·7862	0·7859
Parabola x^2	0·3333	0·3400	0·3350	0·3341	0·3337
$\sin x$	0·4597	0·4580	0·4593	0·4596	0·4596
$\log(1+x)$	0·3863	0·3850	0·3859	0·3861	0·3862
e^x	1·7183	1·7234	1·7197	1·7187	1·7186
$\frac{1}{1+x}$	0·6931	0·6945	0·6937	0·6934	0·6933

(3) If $F(x)=a+bx+cx^2+dx^3$,

$$\int_0^1 F(x)dx=\tfrac{1}{3}\left[F\left(\frac{2-\sqrt{2}}{4}\right)+F(\tfrac{1}{2})+F\left(\frac{2+\sqrt{2}}{4}\right)\right]$$
$$=\tfrac{1}{3}[F(0{\cdot}1464)+F(\tfrac{1}{2})+F(0{\cdot}8536)]. \quad . \quad (b)$$

A simple three-ordinate rule is therefore

$$\int_0^1 F(x)dx=\tfrac{1}{3}[F(\tfrac{1}{7})+F(\tfrac{1}{2})+F(\tfrac{6}{7})].$$

In practice this is not quite so convenient as the application of the two-ordinate rule.

A. F. DUFTON.

Trinity College, Cambridge, May 20.

P.S.—I thank Mr. C. F. Merchant for pointing out in NATURE of June 3 that the four-ordinate rule is already in use, and for giving a reference to Tchebycheff's rules, with which I was unacquainted. The positions of Tchebycheff's ordinates, as in (*a*) and (*b*) above, are inconvenient, and the rules obtained by taking neighbouring ordinates attain simplicity without great loss of accuracy. A. F. D.

June 5.

The Cost of Laboratory Fittings.

IT is evident from the correspondence which has followed the publication of the letter from me on the subject of laboratory fittings that I must again ask leave to trespass on your space in order to explain that my remarks referred solely to fixed fittings, as stated, embracing working benches, lecture tables, and the like. I have no doubt that questions of actual instruments and apparatus are of much greater importance, but of these I have no right to speak.

Perhaps I may be allowed to make myself clear by reference to one or two specific directions in which research on fixed fittings might possibly prove useful. The present price of teak as bought in bulk from a merchant is 30*s*. per cubic foot, and if impregnated soft wood could be substituted for bench and table tops much saving would result. This impregnation might be effected by precipitation, electrolysis, oxidation (oils), or evaporation (*e.g.* silica solutions). Again, bituminous materials with perhaps barytes rolled into them might be investigated for use as a thin layer on wood or concrete. Soapstone is much used in America and *lave émaillée* in France, but not as yet in this country. There are, further, certain hard flooring plasters which should be very inert chemically. An investigation is much needed into the proper composition of bituminous materials for coating laboratory drains. Drains executed in wood thus coated are in many cases much cheaper than glazed ware drains.

For repetition work such as locker doors and even drawers pulped and stamped material might prove economical if some standard could be agreed upon. It should not be a very expensive matter to set on foot some researches of this nature, and any effective results would, I imagine, be very welcome to institutions at present faced with additions to their material equipment.

ALAN E. MUNBY.

9 Old Square, Lincoln's Inn, W.C.2.

The First Act of a Young Thrush.

SINCE observations of the first acts of wild birds immediately after hatching are very difficult, the following may be worth recording. Last week I went to look at a thrush's nest which I had found a fortnight before, with four eggs in it. Two eggs were hatched and two were not. As I was watching the young birds, one of the two remaining eggs cracked right across, and I saw the bird wriggle out and toss the two halves of the shell out of the nest by a convulsive movement of its back; but the curious thing was that, before the bird was properly free from the shell, it opened its beak—as if for food. I dug up a worm near by and offered it to the bird, which swallowed it eagerly. I purposely dug for the worm in a place from which I could see the nest, and I feel sure that the parent bird did not come and feed the nestling meanwhile. A few minutes later the other egg hatched, and the bird behaved just as in the former case, opening its beak before it was out of the shell.

Now the question is: Was the opening of the bird's beak a reflex or an "instinctive" act? If it were reflex, it would presumably have been induced by sudden exposure to the new environment of open air; and, obviously, such a reflex act would serve the purpose of an "instinctive" one in this case. Moreover, is it not a question whether any "instinctive" act at so early a stage can be anything more than a reflex act thus adaptable to survival purposes—by natural selection if need be?

HONOR M. M. PERRYCOSTE.

Polperro, Cornwall, May 30.

Marat and the Deflection of Light.

CARLYLE'S vivid portraiture of Marat as "horse-leech" and savage revolutionist has rather obscured the fact that this "friend of the people" was a learned doctor of medicine, a physicist, and a physiologist. It is true that Carlyle refers to him as "Renovator of Human Science, Lecturer on Optics," but the mistake about the "horse-leech" is repeated in the same passage.

In Marat's "Notions élémentaires d'optique" (1784), p. 16, the following statement is made:

"Il est hors de doute, que les rayons de lumière changent toujours de direction dans le même milieu, lorsqu'ils passent à certaine distance d'un corps. Se trouvent-ils dans la sphère d'attraction? ils se replient jusqu'à certain point à sa circonférence, et se prolongent ensuite en droite ligne."

This at first glance may appear a remarkable anticipation of recent discoveries in physics, but in reality the conclusion is based on wholly false premises, as further reading of the pamphlet will disclose.

W. A. OSBORNE.

University of Melbourne, April 22.

British and Metric Systems of Weights and Measures.

ON p. 355 of NATURE of May 20 Mr. M. E. Yeatman in a letter on the above subject says: "It seems that the advantage of any given system of weights or measures lies largely in the facilities that it offers for the division of a sum or quantity into equal parts"; and I have seen "facility of factorisation" claimed before as one of the merits of the British system. As an engineer who "figures frequently," I fail to appreciate this fetish of factorisation. One uses a slide-rule and logs, and never worries about factors. Will Mr. Yeatman, or someone else, demonstrate the use of factors in practical calculations, bearing in mind the use of slide-rules, calculating machines, and logs?

The metric system seems to be gaining ground in spite of the lack of factors disclaimed for it.

ALFRED S. E. ACKERMANN.

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